

some decompositions of protein, although Kellner's investigations indicate that the animal does not benefit thereby. The decomposition by bacteria is prevented in the stomach by the hydrochloric acid of the gastric juice, and in the lower part of the large intestine by the progressive resorption of water from the intestinal contents. In the small intestine, however, the organisms are more active, giving rise to ammonia, phenols, indols, etc. The two latter are largely resorbed; they are of little, if any, use to the animal—indeed, they are poisonous; they combine, however, with other substances and are excreted in the urine as the so-called ether-sulphates.

Considerable progress has been made in our knowledge of the utilisation of fat. At an early stage in the mobilisation of the reserve in the adipose-tissue cells the fat becomes hydrolysed, yielding glycerol, which, perhaps, serves as a source of dextrose, and a fatty acid, which is oxidised. Dakin and others have shown that the oxidation of the acid begins at the β carbon atom (*i.e.* at the second from the COOH group), and results in the splitting off of two carbon atoms at a time, yielding water, carbon dioxide, and another fatty acid containing two fewer carbon atoms than the original one, with which the same process of erosion is repeated. It is not yet clear, however, how the animal utilises formic, acetic, and propionic acids, although it undoubtedly does so.

The author then proceeds to discuss the various types of experiments made by investigators in animal nutrition: the simple feeding trial, in which the gross gain in body-weight is measured for a particular ration; the digestibility experiment, which requires more careful measurement and aims at determining what proportions of the various food constituents have been digested and resorbed; the "balance experiment," in which respiration determinations are added to the foregoing in order to make up a balance-sheet showing exactly what has become of the food; and, finally, the elaborate calorimeter experiment, in which an attempt is made to trace the energy changes involved.

The author is well known for his investigations on the energy relationships of nutrition, and his own beautiful calorimeter at State College is the envy of many another institution. He devotes considerable space to this aspect of the subject.

Incidentally, he makes an interesting comparison between the efficiency of a horse and that of a power plant. He finds the total useful work done by a working horse was 2.8 therms; the gross energy of the ration was 55.8; the over-all efficiency was, therefore, 5.1 per cent. The animal worked six hours per day. Supposing his bodily machinery was stopped for the other eighteen hours (as an engine would be), and he was charged with only a fourth of his maintenance requirement, the over-all efficiency would be raised to 6.3 per cent.—about that of a modern American locomotive. In actual practice the conditions with an animal are very much as if it were necessary to keep up a full head of steam for twenty-four hours, or to run an

internal-combustion motor continuously, although work is only done for part of the time.

The author attaches less importance than usual to starch equivalents, which he considers may obscure the energy relationships. Altogether the volume is very interesting, and will be read by agricultural teachers with much pleasure.

ULUGH BEG'S CATALOGUE OF STARS.

Ulugh Beg's Catalogue of Stars, revised from all Persian manuscripts existing in Great Britain, with a vocabulary of Persian and Arabic words.

By E. B. Knobel. Pp. 109. (Washington: Carnegie Institution, 1917.) Price 2 dollars.

THIS work forms a sequel to Mr. Knobel's edition of Ptolemy's Catalogue (see NATURE, vol. xcvi., p. 282). Owing to the war he has only been able to use codices existing in England, but these are fortunately rather numerous, and twenty-two Persian and Arabic MSS. have been collated. A partial collation of three Persian MSS. at Paris by the late Prof. C. H. F. Peters has also been utilised.

This catalogue of 1018 stars, the first original catalogue since that of Ptolemy, is founded on observations made during the reign of Ulugh Beg, a grandson of Tamerlane, at his observatory near Samarkand, the epoch being A.D. 1437. It was published in 1665 by Hyde from three codices at Oxford, and this edition was reprinted in 1767 in the collected edition of Hyde's works. It was again issued by Bailly in 1843 in his edition of ancient star-catalogues, in which the stars were for the first time identified and the modern designations given. Mr. Knobel's edition differs from Hyde's not only by being founded on a far greater number of codices, but also by giving the places of the stars for 1437 computed from modern star-catalogues (by Peters) and a comparison of these with Ulugh Beg's places.

No particulars about the instruments employed or the methods of observing are known. Peters was the first to notice that the minutes of the longitudes are generally of the form $3n+1$, while the minutes of latitudes are multiples of 3, as if the circles of the instrument were graduated to $3'$ and some correction of $1'$, $4'$, or $7'$ had been applied to the longitudes. Ulugh Beg states that twenty-seven stars in Ptolemy's Catalogue were too far south to be observed at Samarkand, and that their places were, therefore, borrowed from Ptolemy, allowance being made for precession. Mr. Knobel has found that the longitudes of four other stars were derived in the same way, and were not observed. In addition to these, there are at least eighty-two pairs of stars of which the longitude of one star only was observed, while that of the other (a few degrees distant) was obtained by adding or subtracting Ptolemy's difference of longitude. The latitudes of sixty-eight stars were simply copied from Ptolemy, and there are at least forty-four pairs

of stars of which the latitude of one star only was observed, while that of the other was obtained by means of Ptolemy's difference of latitude. Therefore, the longitudes of only about 900 stars were actually observed, and the latitudes of about 878 stars. But there is a strong suspicion that the original observations should be still further reduced, as there are some forty or fifty stars the errors of the places of which resemble the errors of Ptolemy, and thus suggest a derivation from the *Almagest*. These very interesting results of Mr. Knobel's examination of the catalogue have escaped the attention of all previous historians of astronomy.

The comparison with modern star-places shows that the accuracy of Ulugh Beg's observations was not much superior to that of Ptolemy's. Mr. Knobel reproduces a drawing of an altazimuth from a Persian MS. in the British Museum (a treatise on astronomical instruments), which shows the use of diagonal scales for subdividing graduations. As the MS. dates from A.D. 1700, the influence of knowledge derived from Western sources is not excluded. But as diagonal scales were known to Levi ben Gerson, a Spanish Jew who died at Avignon in 1344, it is very possible that some later Arabian observers may have employed them. Judging from his star-places, Ulugh Beg scarcely did so. We congratulate Mr. Knobel on this completion of the long labours of Prof. Peters and himself on ancient star-catalogues.

J. L. E. D.

OUR BOOKSHELF.

A Chemical Sign of Life. By Shiro Tashiro. (The University of Chicago Science Series.) Pp. ix+142. (Chicago: University of Chicago Press; London: Cambridge University Press, 1917.) Price 1 dollar or 4s. net.

DR. TASHIRO gives a useful and readable summary of the results which he has obtained on the production of carbonic acid in nerve and in seeds by the employment of an ingenious micro-chemical method. He regards the evolution of this gas as a sign of life analogous to the "blaze currents" described by Dr. Waller. The magnitude of the CO_2 production which he observes in nerve fibres has raised doubts as to the exact significance to be ascribed to the results obtained. The author deals with some of the criticisms which his work has evoked.

Morphology of Gymnosperms. By Profs. J. M. Coulter and C. J. Chamberlain. Revised edition. Pp. xi+466. (The University of Chicago Press.) Price 5 dollars net.

THIS important work was reviewed at length in the issue of *NATURE* for August 10, 1911 (vol. lxxxvii., p. 171). The revised edition is in no sense rewritten, but important changes and additions occur, the more important of which are in the chapter on Cycadales and in the bibliography. A supplementary list to the latter adds 150 titles to the 484 of the first edition.

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LETTERS TO THE EDITOR.

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Tidal Energy Dissipation.

MR. STREET is in error in attributing to me (*NATURE*, October 25, p. 145) the "contention that viscous action in a solid earth cannot be an appreciable cause of the slowing of its rotation." I have never made any such assertion, it is opposed to my personal opinion, and in the present state of knowledge it is quite impossible either to affirm or to deny a statement of such definiteness. His criticisms of the law of viscosity used by me (*M.N.*, *R.A.S.*, vol. lxxvii., pp. 449-56) are confined to its precise mathematical form, to which I attach little importance, and do not touch the physical conceptions underlying it, which are fundamental. The mathematical argument was only a numerical illustration of the order of magnitude of the effects to be expected from these.

The theory that viscosity in the solid earth is the cause of the lunar secular acceleration requires its effect to be considerable for variable stresses with periods of the order of a day. If, then, the viscosity is of such a character as to permit an indefinite flow when a constant stress is applied for a long enough time, then for stresses with a period of a year or more the substance will have time to flow like a liquid, keeping approximately the hydrostatic form throughout the changes. Hence the Eulerian nutation, a long-period vibration depending for its existence entirely on solid rigidity, could not persist. Similarly, annual variations in the distribution of mass over the surface would be compensated by internal flow, and there could therefore be no annual variation of latitude.

If, on the other hand, the viscosity is not of a type that permits indefinite flow, the strain when a constant stress is applied must tend to a finite value, and afterwards remain approximately constant. The effect of viscosity must then be limited to the initial stage. In the case of a periodic stress the period of which is long compared with this initial stage, the rigidity will be of much more importance than viscosity, and the substance will behave nearly as if perfectly elastic. On the other hand, if the period is short in comparison, viscosity will be of greater importance. This is supported by the fact that if the viscous forces are directly proportional to the rate of straining, as is inherently probable on account of the analogy to electric resistance and fluid viscosity, the same is found to hold. On such ideas the law I called that of "firmo-viscosity" is based. If, then, the effect of such viscosity is considerable when the period is twelve hours, it must be more important than elasticity when the period is only a few seconds, as in the case of earthquake waves. Thus the transmission of these waves would be prevented. It follows that firmo-viscosity is absent from the earth so far down as seismic waves travel; it may, however, be important at still greater depths.

If on the application of a constant stress to a body the strain at once assumed a finite value, then slowly increased for a few days, and afterwards remained constant, the viscous properties of such a body would bear a close resemblance to those of the earth as a whole. In this case, however, the rigidity found from the Eulerian nutation should be much less than that found from earthquakes, which does not appear to be the case. This suggestion, therefore, alters the difficulty without removing it.